

XL.—*Some Anticriticisms.*

By ERNST HARTERT, Ph.D., F.Z.S.

To call attention to and to rectify all errors in ornithological literature is neither possible nor, unfortunately, always appreciated by the corrected party. If, however, one is inadvertently and erroneously accused of careless mistakes, one must sometimes set matters right, because errors contained in positive statements are more likely to be propagated and will cause other errors. If, moreover, the whole system for which one is fighting and working is attacked in a review, one must reluctantly answer. These considerations have caused the following anticriticisms.

I.

In 'Bull. B. O. Club,' xii. p. 83 (June 1902), Mr. Dresser says that some eggs of *Ammomanes phœnicuroides* "belonged to the form recently differentiated and described by Mr. Hartert (Bull. B. O. C. xii. p. 43) under the name *Ammomanes cinctura zarudnyi*." This means nothing more or less than that I have described *Ammomanes phœnicuroides* (which I treat as a subspecies of *A. deserti*) as a new subspecies of an entirely different species of *Ammomanes*! I need hardly say that I am sufficiently acquainted with the species of *Ammomanes* (one of my favourite groups of birds) to avoid such an error, and that there can be no other reason for Mr. Dresser's statement than the fact that Mr. Zarudny collected examples of both species (*A. deserti phœnicuroides* and *A. cinctura zarudnyi*, erroneously spelt *zarudni* by Mr. Dresser) in the same districts of Eastern Persia, where they live close together, as do other forms of *A. deserti* and *A. cinctura* in North Africa.

II.

In 'The Ibis,' 1903, p. 593, curiously enough, Colonel Bingham charges me with a similar offence, viz.: that I have described a known species as a new subspecies of quite a different species. He says:—

“51. *HEMIXUS HOLTII* (Swinh.).

“This is the species, I think, separated by Mr. Hartert as a subspecies of *H. tickelli* under the name *H. tickelli binghami*. It agrees fairly well with specimens of *H. holti* in the British Museum.”

The case, however, is quite different. Three years ago we received from Col. Bingham a bird which he had named *Hemixus maclellandi* and had recorded under that name in the Journ. As. Soc. Beng. lxi. p. 111 (1900). This bird was, nevertheless, not a *Hemixus maclellandi* at all, but a form of *Hemixus* (or *Iole*) *holtii*, differing from the typical *H. holti* in various details pointed out by me in Nov. Zool. 1902, p. 558, where I described the specimen in question under the name

IOLE HOLTII BINGHAMI,

but not as *Iole tickelli binghami*! It gave me great pleasure to name a bird in honour of one of the best field-ornithologists known to me, and I believe it was the first time that a bird had been named after Col. Bingham. I only regretted that I had no occasion to associate a more strikingly different bird with his name, but still more do I regret now that my work, instead of avoiding a mistake for the future, has led Col. Bingham to make a still more erroneous statement.

III.

In ‘The Ibis,’ 1904, p. 291, appeared what was apparently meant for a review of the first part of my book ‘Die Vögel der paläarktischen Fauna.’ This review is of such a nature that I cannot refrain from answering it—not of course (as my brother-ornithologists will understand) to defend my person, but to defend the system for which I fight, for the sake of truth and the progress of our beloved science.

“It is quite time that a protest should be made against” * reviews in which books are objected to because they are not “conservative” enough, and reviews which only or mostly deal with the nomenclature of a book, while nomen-

* Cf. ‘Ibis,’ 1904, p. 292.

clature (though first striking the eye in the headings of the species) is only a minor detail and not the gist of science. To be conservative in principle is not scientific. We cannot arrest the progress of science and nomenclature, and we must alter our views when we learn new facts and know better.

The "Editors" compare my treatment of species and subspecies with that of Mr. Dresser, whom they "praised for his steadfast adherence to the old-fashioned binomial system of nomenclature," and with whom they agree because "even he recognises subspecies in certain cases." If, however, the "Editors" had gone into details and had studied some of the cases in question, they would have found that just the fact that Mr. Dresser had recognised certain subspecies and neglected others is the weakest point in his book and makes it a very misleading mentor. Why, for example, has Mr. Dresser recognised the various forms of the Dipper, when, on the other hand, he has passed over in silence more than a hundred other forms which are equally or even more distinct? That is a purely arbitrary proceeding, and therefore not scientific. It is true that Mr. Dresser ends his book with the sentence: "Subspecies described under trinomial titles I have not considered it necessary to be included"; but is *that* a scientific method? My opinion is that they should only be passed over after due consideration of their value, but not because they were "described under trinomial titles." Such due consideration they have not received in Mr. Dresser's 'Manual'—they were not quoted because they were "described under trinomial titles." Thus the synonymies in the 'Manual' are incomplete and almost useless, as one does not know which forms, inhabiting which countries, have been named. But also many forms described under binomial titles have not been duly considered in the 'Manual,' or else such remarks as on p. 886, that *Asio canariensis* Mad. is not separable from *Asio accipitrinus*, while it is a most distinct form of *Asio otus*, or that *Stric ernesti* Kleinschm., which is by far the whitest form of Barn-Owl, is a "dark race" of the latter, could not have

been made. Other names are not even mentioned, though described binomially. No doubt this is the praised steadfast adherence to the "old system," which allowed only one form of the Barn-Owl and only one of the Long-eared Owl. But the majority of ornithologists are no longer content with the "old system." Science has progressed in rapid strides within the last twenty years. We all know now that these Owls, as well as most other birds, are not the same everywhere, but that they are easily separable into various geographical forms. The study of these geographical representatives—or subspecies, as they are now, somewhat unfortunately, called—is scientifically of the same importance as that of the widely different species, and neither Mr. Dresser nor the Editors of 'The Ibis' will be able to stop the progress in that direction, whatever they may do. If this is admitted, and I am sure it is not necessary to explain the importance and the necessity that local forms should be studied, then we must also have names for them, in order to talk of them; and it is most unfortunate that some of the leading British ornithologists still refuse trinomials for them! What can be more simple than calling all the Crested Larks (except those belonging to *G. theklae*, a distinct species living in the same area as some forms of *G. cristata*) *Galerida cristata*, adding a third name when the various local races are discussed: *Galerida cristata pallida*, *Galerida cristata riggenbachi*, *Galerida cristata nigricans*, *Galerida cristata cristata*, *Galerida cristata macrorhyncha*, *Galerida cristata arenicola*, &c.? What serious objection can be made to this very simple method, which leaves it open to everyone to use binomial or trinomial names—trinomials if the local forms are discussed, binomials if the broad facts (species) only are recognised? This choice to use binomials or trinomials is one of the advantages of our system, every trinomial being easily reduced into a binomial, while the other method muzzles us and forces its contentions on to us without choice.

But what was and what is the praised "old system"? Let me quote instances from the last-named group, that of the Crested Larks, so as to remain at one subject.

When Dr. Blanford described a new "variety" of Crested Lark from Abyssinia, in 1870, he called it

"*ALAUDA (GALERITA) ARENICOLA*? Tristram, var. *FUSCA*."

I suppose that was the old system.

I would call this form (if I could separate it)

"*GALERIDA THEKLE FUSCA*."

That is with three names (easily reduced to two) instead of six words and a comma.

And what has Mr. Dresser done with the Crested Larks? In the 'Manual' he has recognised two Crested Larks, calling them

"*CORYDUS CRISTATUS*" and

"Subsp. *CORYDUS ISABELLINUS*."

I suppose that is another form of the "old system."

Let us, nevertheless, see what it means, for it is full of mistakes. First of all, two distinct species, *G. cristata* and *G. theklæ* are lumped. Secondly, one out of about ten equally distinct subspecies is recognised, and this arbitrary proceeding is backed by the bold statement that "this species is subject to considerable individual variation both in colour and size, and has consequently been greatly subdivided by modern ornithologists." This statement, however, apart from the insinuation that "modern ornithologists" name individual aberrations, is a dangerous misrepresentation of facts, because there is, on the contrary, very little individual variation in the Crested Larks, the variation being connected with the "habitat" and geographically limited! If Mr. Dresser had not made his erroneous statement, and if he had united all Crested Larks, saying, for example, "Adhering to the old-fashioned method of only recognizing broad facts in nature, disregarding geographical races and troublesome details, I only recognize one species, which I call *Galerida cristata*," then we should be able to understand him. There would then only be one mistake, the uniting of *G. theklæ* with it, which we cannot understand, since equally and even more similar species of *Phylloscopus*.

Acrocephalus, and other genera are readily recognised in the 'Manual.' Admitting *G. isabellina* the author spoils everything—and where is the binomial system if he says "subspecies *Galerida isabellina*"? Is that shorter than *Galerida cristata isabellina*?

There remains another method, that of recognising all, even the most closely allied geographical forms and naming each with two names. This is Dr. Sharpe's method, forcibly brought before us in his 'Hand-list,' but it is most objectionable and disturbing. If I recognise *Galerida cristata* and *Galerida theklæ* as two species, subdividing each into a number of subspecies, it is clear to everyone, and illustrates at a glance a most important fact: what forms agree in their main characters, differing in certain details connected with geographical separation, and what (though they may be superficially similar) belong to totally different species inhabiting similar areas. Dr. Sharpe's method hides all this, and moreover raises objection and dissent. Ornithologists cannot be forced to allow as species, binomially named, two forms differing merely in the bill or wing being on an average two or three millimetres longer, while nevertheless such facts are not without significance and should not be overlooked. One might therefore separate such closely allied geographical races as subspecies, but general consent can never be obtained to treat them as species binomially named.

The Editors of 'The Ibis' confront my "four names" with Dresser's "two names," saying that they prefer the Raven being called "simpler and shorter" *Corvus corax* instead of *Corvus corax corax* L. It is probably not meant seriously to call the author's name a fourth name! To add it to a specific name is an old custom among zoologists and botanists. It can do no harm and is often very useful: moreover, it can be left out by all who do not care for it, and it is therefore not a burden to nomenclature. The whole phrase looks like a *captatio benevolentiae* of the readers, and is not quite correct. I, too, call the Ravens *Corvus corax*, but when I distinguish between the various

geographical forms I call the first-named one *Corvus corax corax*, repeating the specific term rather than using a new name “*typicus*” for the same. This is a mere detail and everyone can easily say “*typicus*” instead—my book does not prevent anyone from doing so. But what, again, does Mr. Dresser do? He recognises *Corvus corax* and *Corvus tingitanus*, passing over the equally distinct *C. hispanus*! That is again erroneous. It is not a question of simpler nomenclature, but a question whether we should study allied forms closely or follow preconceived ideas, uniting most or as many as we please of the geographical forms.

The Editors of ‘The Ibis’ hold me much to blame for preserving the original gender of those specific names which appear in the form of adjectives. In my opinion, the way towards a stable nomenclature is that of preserving the original spelling entirely, and to regard all names merely as names, not as adjectives in connection with the genera as substantives. This will go far towards uniformity. Otherwise there will be more doubtful cases than one may think. There is already a difference of opinion whether substantives like *piscator* and *sibilatrix* should alter their gender into *piscatrix* and *sibulator*, if connected with a genus of the other gender. Then there are many words the gender of which is doubtful and often wrongly accepted. There is the well-known term *Nucifraga*, evidently of masculine gender, meaning the Nutcracker, but universally treated as of feminine gender. There is *Ammomanes*, of Greek derivation (from *ἄμμος* and *μαίνομαι*), a word ending in *ης* and therefore masculine, yet always used as a feminine; there is *Halcyon*, generally looked upon as a feminine, yet in the ‘Catalogue of Birds’ a masculine. On the other hand, the gender of most generic names is clear to every schoolboy, and it is an easy matter for all ornithologists who care for it to show that they have been at school and to alter the original gender, as preserved in my nomenclature, in accordance with their classic feelings. My book shews the original spelling of every name, and it is therefore useful to all those who care for strict priority, while nothing prevents those who

are less particular from altering the gender of some of the specific terms.

It would seem that the Editors of 'The Ibis' have only glanced at some of the headings of my species and subspecies when they say that "the main point of the book is that the author calls upon us virtually to give up the binomial system." Alas! poor book, it had better have remained unwritten if there were no other points of more importance in it; but I am not modest enough to agree with the Editors of 'The Ibis.' Every genus in my book contains a "key" to the species which I recognise as such, and the names of all of them are binomial! Of course, my "*Corvus corax*" includes all the various races of the Raven, also the North European race, *Corvus corax corax*. It is, in my opinion, quite illogical to call one race out of half a dozen by two names, merely because it was the one named first, all the rest by three, merely because they were named subsequently. To repeat the specific name is decidedly simpler than any other method; I have tried them all, and my method is rapidly gaining ground: in the last ornithological number of the 'Tierreich' it is adopted, the Americans (Ridgway) have at last accepted it, &c. Moreover, of the 394 forms described in the first two parts of my book, about 120 are called by binomials, all those of which no geographical races are known. Surely that is not giving up the binomial system! On the contrary, I retain it throughout, merely supplementing it by trinomials where it is desirable.

For "joining together in one genus the Goldfinches, Siskins, Redpolls, and Linnets" I have given full reasons, showing the fallacy of former treatments. Of course the Editors of 'The Ibis' have the right to stick to their own ideas—and ideas about genera are generally differently interpreted and changeable,—but they are in error if they believe that in this case they have caught me slipping. I have not overlooked the generic name *Carduelis*. They ascribe it to "Schaeff," but the author's name is not "Schaeff," but "Schaeffer," abbreviated into "Schaeff." according to custom. Though he was certainly not a sheep

(Schaeff), but a shepherd (Schaeffer), his names are not admissible, because he did not use binomial nomenclature. This can at a glance be seen on pages 25, 26, 32, 46, and others; moreover, he took most of his names from Brisson.

If the Editors of 'The Ibis' call "*Pica pica pica*" a monstrosity, what is their opinion about

"*ALAUDA (GALERITA) ARENICOLA* ? Tristram, var. *FUSCA* " ?

Unfortunately, I have still one more point to argue. The Editors have openly challenged me, asking: "Can Mr. Hartert say that if British skins of these birds were mixed up with some of their continental representatives, he would always be able to pick them out?"

Why was this question put? Evidently with the idea that one should always be able to pick out the various forms which one recognises, and with the supposition that the Editors—or I may say the senior Editor, because the junior Editor has not named new species or written monographs of difficult families of birds—can always easily distinguish ("pick out") the species they recognise, or at least those which they have described themselves. Unfortunately these views are both fallacious. The question was dangerous, and the arrow from their bow is springing back to the shooters. I will only quote two examples. In Cat. B. xiv., *Muscisaxicola albifrons* Tsch. has been redescribed as *Tanioptera holospodia* Scl., though specimens of both were to hand; in Cat. B. xv., *Pyriglena serva* Scl. and *Cercomacra hypomelena* Scl. are described in two different genera, yet they are quite the same, absolutely indistinguishable. Where have I done a similar thing? Moreover, I am convinced that I shall, as a rule, be able to pick my new subspecies out easily if put to a fair test, although I myself do not demand it, nor expect it in all cases for all future. In the introduction to my book I have explained that it is no longer the goal of ornithological studies to "name" every single individual and to put the "correct name" on the label. The most important thing is to find out and to

interpret facts, and if we do this we shall often find that an excellent geographical form, evident at a glance when confronting two series, contains single individuals which do not follow the rule, but are intermediate or do not represent the various characters by which the two forms can generally be distinguished. Therefore, though I certainly require that two species should be distinguishable, I do *not* require that each individual of every geographical form ("subspecies") should at once be distinguished. If thirty specimens of a British bird are distinguishable from thirty from the Continent of Europe and one is not, then I must recognise the two forms as subspecies. The one which does not follow the rule may be intermediate or aberrant, though generally it may only be a straggler from the other country, but it cannot give us the right to overlook the fact that there are two different forms.—*Sine ira!*

XLI.—*Note on Tanysiptera dea.*

By Count T. SALVADORI, F.M.Z.S.

MR. OTTO KLEINSCHMIDT, in a very curious paper on the "Ornis von Marburg an der Lahn" (Journ. f. Orn. 1903, pp. 440-507) has already shewn (p. 461) a strong case of the inconvenience of going back in nomenclature to the tenth edition of Linné's 'Systema Naturæ' (1758). In the edition of 1758 the description of *Turdus iliacus* (p. 168) is that which fits *T. musicus* (*alis subtus flavescentibus . . . linea nulla superciliarum alba*), while the description of *Turdus musicus* (p. 169) is evidently the one which fits *T. iliacus* (*alis subtus ferrugineis, linea superciliarum albicante*). Linné, in the twelfth edition of the 'Systema Naturæ,' corrected the mistake that he had made in the tenth edition, and *T. iliacus* is there described as follows:—"alis subtus ferrugineis, superciliis albicantibus"; while the description of *T. musicus* runs as follows:—"remigibus basi interiore ferrugineis,"